JAUNDICE

FROM

NON-ELIMINATION,

TOGETHER WITH REMARKS ON THE PATHOLOGICAL CONDITION AND CHEMICAL NATURE OF THE BILE.

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During the time I was resident in the Royal Infirmary of Edinburgh, my attention was chiefly directed to a subject, which of late years has attracted some attention as an object of pathological inquiry. The disease to which I allude is Jaundice from Non-Elimination. But in entering on the consideration of this subject, it will be necessary briefly to advert to some other forms of jaundice, arising from causes more demonstrable, and better ascertained.

The sudden appearance of jaundice, arising without premonitory symptoms, or supervening on some other and long-continued disease, was usually referred to a doctrine prevalent some years ago, and even supported by some in the present day, viz. spasm, or the very opposite condition, paralysis of the gall-ducts. This theory is so purely hypothetical, and so little supported by direct evidence, that I am constrained to regard it as a mere loop-hole, of which the older physiologists were accustomed to avail themselves, and to which they were driven by the erroneous idea that the liver was the sole agent employed in the

formation of bile. (Bostock, Phys. p. 370). The observations of physiologists go far to show the muscularity of structure in the middle coat of the bile-ducts; but, granting that their reasoning were founded on firmer grounds than those of analogy, the supporters of the doctrine of spasm have yet to prove that the muscular contractility is actually capable of being excited into action by merely mental impressions. I do not deny, nevertheless, that the presence of gall-stones may by direct contact produce irritation, and consequent contraction of these tubes, as Rudolphi, Monro, Tiedemann, and Müller, have shown that, by such direct application of stimuli, contraction may be distinctly but slowly produced. This is, however, denied by Saunders. At the same time, the existence even from this cause of these spasms (les coliques hepatiques) appears to rest more upon hypothesis than actual observation; and it is remarked by Bouillaud, that in those instances in which biliary concretions have been found after death, the patients had never previously complained of any uneasiness, or given any indication of their presence, (Arch. Gen. de Med.); and this is fully corroborated by Andral (Clin. Med. Tome iv.) The opinions of Abercrombie on this point are probably familiar to all; and I only allude to them as agreeing with those already expressed.

The next cause of jaundice, which I shall notice with equal brevity, is where there exists a "mechanical obstruction to the flow of bile, already secreted," as in the case of gall-stones in the ducts; a loaded state of the colon (Dr Abercrombie); inflammation of the mucous membrane of the duodenum, extending to the mouth of the common duct, and causing obliteration, (Dr Marsh); the occasional existence of abdominal tumours, enlargement of the pancreas, &c.; and I may here mention the well-known case of Dr Saunders, in which some gooseberry-seeds proved the cause of obstruction. The ducts being thus obstructed from any cause similar to these enumerated, an accumulation of bile takes place, and a gradual regurgitation within their canals, while the absorbents of the liver, stimulated into increased action by the presence of the fluid, the lately secreted substance is again taken into the circulation, producing the deep-yellow colour so peculiarly characteristic of the disease. (Ferrus, Dict. de Med.) That absorption does take place in this way, and under such circumstances, is, I think, shown by the experiments of Dr Saunders, and also by one of my own, to which I shall presently advert.

At first sight the difference between these causes referable to mechanical obstruction, and those in which the process of elimination appears to be merely suspended, seems one of little importance; and the result being in either case the impregnation of the blood with bile, such a distinction may seem more curious than useful. But further examination will show, that fatal conse-

quences are far more common in cases where jaundice proceeds from non-elimination, than in those where the biliary matters have been secreted, and subsequently reabsorbed. The only explanation of this physiological fact is that offered by Professor Alison, (Ed. Med. and Surg. Journal, Vol. xliv.) who advocates the opinion, that, in the process of reabsorption, the biliary matter undergoes some change, which renders it less injurious to the animal economy; and quotes in support of this idea the assertion of Dr Prout, that nothing is absorbed into the living body without having previously undergone a process analogous to that of digestion, which idea is strongly supported by the great similarity of the contents of the lymphatics at different times and in different parts of the body, as ascertained by Magendie. The greater facility also with which some substances, and the difficulty with which others are absorbed into the body, appears to me to offer an additional explanation; because, while the inert colouring matters are readily taken into the system, the poisonous resin or cholesterine may be absorbed only in small proportion. Either of these views will, I think, suffice to explain the more frequent occurrence of coma and death in cases of jaundice from non-elimination, than in cases where we have reason to believe that the biliary matter has been eliminated, but again absorbed.

With these remarks on jaundice in general, I shall now turn my attention to that particular form of it which has been termed jaundice from non-elimination. This suspension of the biliary secretion, it will be necessary to state, may be dependent either upon disease of the liver itself, whereby that organ is rendered unfit for the performance of its ordinary functions, or it may take place, as there is every reason to believe, spontaneously, and without any appreciable lesion of that organ. Hence a very obvious division of the subject. To the first class belong those cases in which there exist inflammation and congestion of the liver, abscesses of the liver, scirrhous or malignant diseases in general, or fatty degenerations in the gland. All these diseases may, however, take place to a very considerable extent, without altering the appearance and physical conditions of the bile; and this secretion may, on the other hand, be obviously vitiated without any marks of disease in the liver. (Andral).

With regard to inflammation of the liver, it has been remarked by Dr Abercrombie, that jaundice is most frequent when the inflammation is situated on the concave surface of the liver. Annesley, Cheyne, and many others, have remarked the frequent occurrence of jaundice in fever, and indeed every one conversant with practice must be aware that its occurrence under such circumstances is by no means uncommon. Local congestion of internal organs in cases of fever is, as every one is aware, ex-

tremely common ; hence, congestion of the liver, at least in some instances, might naturally be looked for, and this in yellow fever is actually the case,—the liver being usually found to be distended with adventitious matters. The same is doubtless frequently the case in the fever of this country, though, perhaps, in a less severe degree. The yellow gum, as it is popularly termed in Scotland, or the jaundice of new-born children, has been referred by some, (Dr Powell, Dr Burn, &c.) to a too viscid state of the bile ; an opinion I have not thought it necessary to discuss. Breschet has with more reason attributed it to the congested state of the liver; and the observations of Bouillaud, Andral, and Billard, show that this organ in new born infants is frequently, if not generally engorged and distended. I may remark, that it is not always easy to distinguish between the congestion of some authors and the inflammation of others,—those of congestion of Andral bearing much resemblance to those of inflammation of Abercrombie.

In illustration of those cases where the suspension of the biliary secretion takes place, without any appreciable disease of the liver, I shall briefly insert the following

Case.—Jane Stephenson, aged 30, admitted into the Royal Infirmary, October 1839. This patient was admitted for the relief of some pectoral disease under which she laboured, and had formerly been treated in the same hospital for rheumatism. A short time after her admission she was seized with jaundice, and after a few days died in convulsions, produced, there can be little doubt, by the poisonous influence of biliary matter upon the brain.

On examining the body, all the tissues were found of a deep yellow colour, and this especially observed in the cartilages of the articulations. The heart and membranes were a good deal diseased. The liver was small, especially the left lobe ; it weighed only one pound eight ounces ; it was firm, and without any apparent change in its textures, and presented a yellowish tinge. A probe introduced into the *ductus communis choledochus*, and passed downwards, entered the duodenum readily. On tracing the bile-ducts into the surface of the liver, they were found pervious throughout and empty of bile. The kidneys were healthy.

For cases of a similar nature I may refer to those related by Abercrombie, Marsh, Alison, (Med. and Surg. Journ. Vol. liv.,) and, in particular, to a paper by Dr Griffin in the Med. Gazette for 1833—34. There is also in the same volume another case related by Dr Aldis. In the cases mentioned by Dr Griffin, this fatal form of disease appears to have prevailed amongst several members of the same family ; one only recovering from it. In all these cases, as well as in many occurring under the observation of Mr Twining in India, " no cause could be assigned for the

disease." The opinion, that jaundice, in some cases at least, is dependent simply upon a suspension of the ordinary function of the liver, appears to have obtained, in the time of Boerhaave and Morgagni; and it is somewhat amusing to observe, that Dr Saunders only alludes to it to remark, that such a theory is so little tenable as to render it unnecessary to refute it. It is upon suspension of the biliary secretion that I would explain the case I have related, as well as those mentioned by the authors above-named, as in these cases no disease of the liver could be detected on examination, while the retention of the biliary matter in the circulation was evidenced by the yellowness exhibited, and by the coma and convulsions which preceded death. To this cause I would refer those frequently cited, of patients who have suddenly become jaundiced after some sudden shock or mental emotion. I might quote the well-known instance of this kind, in which a young man died at La Pitié, deeply jaundiced, in consequence of a musket having been pointed at his breast. Even where gall-stones have been found after death in cases of this kind, it appears more than probable that, at least in some instances, the jaundice was coincident, and not consequent, upon the existence of these concretions.

To admit the existence of this suspension of secretion is to recognize an important physiological principle, viz. that the blood is the general material from which every secretion, however complicated in its nature, is derived, and that the office of the glandular system is not to secrete, but merely to eliminate from the general mass, at least the elements of the various secreted fluids. The great variety of substances which have been at different times and by different chemists detected in the blood, tends to give the highest probability to this theory. Cerebrine, for example, the peculiar and principal constituent of the brain; albumen, fibrine, the base of muscle, salts, biliary and fatty matters, have been shown by Prevost, Dumas, Chevreul, and others, to exist in the blood; and in an experiment of my own, performed upon a dog, where the *vena portæ* was tied, scales of cholesterine, visible to the naked eye, were found in the blood of the heart, and especially in the fluid of the abdomen. The actual observations of authors upon the condition of the blood in jaundiced persons will here be of importance in guiding our speculations; and I gladly avail myself of those collected by M. Le Canu, (Etudes Chemiques sur le Sang Humain). "Parmi les chimistes," says he, "les uns pretendent que le sang des ictériques renferme toujours de la bile, d'autres, au contraire, qu'il n'en contient pas, et doit sa couleur à la presence d'une matière colorante particulière; d'autres enfin adoptant une opinion, en quelque sorte mitoyenne, pretendent que sans contenir de bile, le sang des ictériques contient

ses principes colorants. Parmi les premiers, on compte MM. Orfila, Clarion. Parmi les seconds, on compte MM. Thenard, Lassaigne. Parmi les derniers, on compte MM. Chevreul, Collard de Martigny, F. Boudet, Le Canu. To the latter I may also add MM. Solon, (Bull. de Therapeutique.)

Admitting, however, that these various substances have been found in the blood, the process by which they are converted into the different secretions, as bile, saliva, urine, &c. eludes at present the most careful scrutiny. It appears to me probable, that while we are right in refusing to the glandular system the power of converting the blood into the several products of that system, the elements of those substances may, in passing through the glands, be combined in consequence of some peculiar influence there exerted, and the result be the substances known to us, as bile, saliva, or any other. It was in connection with this subject that I made the following experiment.

I took a mongrel dog of moderate size, and, with the assistance of Mr Spens, placed a ligature upon the *vena portæ*. A vessel, which, at the time, was believed to be the hepatic artery, was also secured. The protruded viscera were then returned, and the wound brought together by means of sutures. The dog on being released leaped from the table and shook himself. Three hours after I visited him, when he recognized me, answered to my call, and showed a desire to follow me. Eight hours after the operation I again saw him. He answered to my call, and was able with some difficulty to follow me to my residence, at a short distance from the place where the operation was performed. At this time the conjunctivæ were slightly injected, but not at all yellow. Twenty-two hours after the operation he was very stupid and sleepy, gave no indications of pain, and did not respond by any motion when called; he had taken no food, and had not passed either fæces or urine. He was found dead in an easy posture thirty hours after the operation. On examination, the conjunctivæ were perfectly white, as were also the cartilages of the ribs and integuments of the chest. The liver was deeply tinged with bile, as was also the whole of the abdomen. The gall-bladder was distended. The ligature upon the *vena portæ* was rather loose, but appeared to have answered the purpose of arresting the flow of blood, as there was a firm coagulum within, and a little purulent matter. Instead of the hepatic artery being tied, it was found to be the *ductus communis choledochus*, and hence the distension of the gall-bladder. The blood of the inferior *venæ cavæ* and other abdominal vessels contained floating particles, easily seen by the naked eye, which I was able to pronounce to be cholesterine. The vessels being tied above and below, the heart and lungs were then removed, and washed carefully in cold water. On opening

the heart, the blood contained had an oily appearance, and minute scales were visible on the surface. As the blood of the heart had lost nearly all its serum, I added distilled water at a temperature of about 96° ; after some time, small scales, scarcely visible to the naked eye, were observed to float upon the surface, with a larger one in the centre, which could be distinctly recognized as cholesterine, and, on placing a drop containing these scales under the field of an excellent compound microscope, no doubt remained as to their nature, and I did not deem any further examination of them necessary. The blood and fluid of the abdomen contained, as I have already said, a still larger proportion of this substance. The bile, which appeared healthy in its characters, contained, when chemically observed, the ordinary quantity of cholesterine.

This experiment, though on the whole satisfactory and confirmatory of the views I have maintained, is nevertheless open to numerous objections. The common duct was secured instead of the hepatic artery; hence M. Denis, who denies the formation of cholesterine in the blood, might contend for its having been absorbed. It is, however, nearly evident that the absorption of the biliary matter did not take place to any great extent. The examination was not made until some hours after death, and should have been much earlier performed. The experiment would also have been much more satisfactory had I abstracted blood from the jugular vein during life, and after the symptoms of stupor and coma had become evident.

M. Simon de Metz found it impossible to perform the foregoing experiment on the dog, which is certainly one of great difficulty. I repeated it on a subsequent occasion, but the animal died too soon after the operation to draw any conclusions from it. M. S. de Metz, however, seems to have arrived at some interesting and well supported facts in the course of his experiments performed chiefly on pigeons.—(Annales des Sciences, Tom. xiii.) In these animals which do not possess a gall-bladder, there are two hepatic ducts, one of which pours out bile almost continually ; the other, which opens into the great intestine, is generally found empty. If these ducts be tied, the liver becomes engorged with green biliary matter, which soon pervades the neighbouring tissues, the intensity of colour depending on the length of time the animal survives. There is one remarkable fact observed in this case, viz. that, about twenty hours after securing the hepatic ducts, the cloaca was observed in all instances to be filled with green matter similar to that seen in the distended liver, while the intestines contained none. M. S. de Metz considers that this green biliary matter was secreted by the urinary organs, which took on an increased action, at the same time that the natural secretion of these organs had undergone an alteration in its nature ; thus strikingly corroborating the observations of Prevost and Dumas,

that, in cases of diseased kidney in the human subject, the secretion of the liver is not only increased in quantity, but altered in its nature. In five cases of granular disease of the kidney which I examined in connection with this subject, the contents of the gall-bladder were in them all plentiful in quantity, and certainly altered in their nature, especially with regard to the proportion of red colouring matter which they contained.

It naturally becomes a question as to what is the poisonous principle contained in the bile; and the experiment of Magendie, who injected seven grains of bile into the veins of a dog, proves nothing on this point. It does not appear probable that the colouring matter is in itself the injurious principle, as we frequently see persons deeply jaundiced who yet suffer little or no inconvenience from it. I have examined the contents of the gall-bladder, in some instances tolerably well filled, and found that the secretion consisted almost entirely of colouring matter and mucus, but no cholesterine, and very little resin. Absorption taking place, therefore, under such circumstances, would not, I conceive, be immediately injurious, as while the proportion of colouring matter was increased, so as to tinge the whole body, it would yet furnish no criterion of the presence of the more poisonous biliary matters. With regard to the cholesterine, its existence in the blood has been asserted and denied by equally able chemists. I have myself examined the blood of two persons with a view of ascertaining its existence, without being able to detect it, and in the bile of five healthy individuals, (two of them being men killed while in perfect health), cholesterine was wanting in two. I am therefore inclined to believe that not only is this substance occasionally wanting, but that its elimination is usually synchronous with its production. If it were found in the blood in such proportion as it appears to have been in the experiment on the dog which I have related, it would, I think, be rapidly fatal, merely from the mechanical obstruction which would be caused to the circulation by this scaly and peculiar substance. In the bile of the pig, which in many respects resembles closely that of man, I have never observed it; in that of the ox and sheep, it is in very small quantity, while in that of the dog and man it is in general very abundant.

I have now endeavoured to take a hasty and general view of a somewhat extensive subject; but, as I wish to add some remarks on the pathological conditions of the bile and its chemical constitution, I must leave this part of my subject thus imperfectly discussed.

Such observations as we possess on the morbid and healthy conditions of the bile, as found in the human body, are certainly extremely meagre, and, from the fewness of their number, and their frequent want of relation to any ascertained and previous

condition of the patient, can do little towards establishing any mutual connection between disease and the state of this secretion. Besides observations upon the bile taken from the human body in more than thirty cases, I have repeatedly examined the same fluid in the ox, dog, pig, sheep, and rabbit, both with a view to their physical properties, and to their chemical nature ;—and first, with regard to its physical characters. The colour of bile taken singly, can hardly be said to furnish any just criterion of its healthy condition, yet, in conjunction with other circumstances, it furnishes one of the best evidences of the vitiated or normal state of this secretion. Dr Baillie thought that the differences in colour which this fluid presents ought not to be regarded as indications of disease, because they are so commonly met with ; but when we reflect how rarely we have the opportunity of examining the state of this secretion in healthy individuals, this argument can have little weight. Healthy bile is described (I believe, most improperly,) by many as a green fluid,—but it has already undergone some change when it presents this colour. Mr Marshall mixed thick black bile with vinegar, and formed a green mass, like chopped spinach, (Dr William Thomson). Dr Powell also mixed yellow bile with gastric juice, and it became green ; and I have myself observed that bright yellow bile, recently vomited, rapidly became green by exposure to the air, probably from the same cause. But that some variety of colour may exist without morbid alteration seems probable, from observing the bile of two oxen, killed at the same time, in one of which it was green, in the other of a reddish colour. In four cases, in which there was no reason to suspect disease, the bile of man was, " en masse," of a nearly black colour, staining white paper of a " bright orange-yellow." (Werner's Nomenclature). The bile of the ox is of a green colour, but occasionally of a reddish tinge ; that of the dog exactly resembles that of man in appearance. The bile of the pig is of a light reddish colour, resembling sherry wine when diluted ; that of the sheep is dull olive-green ; of the rabbit, light reddish-brown. These characters are constant, and easily distinguish the one from the other. The effect produced in the colour of all these by the addition of an acid is also remarkably different. On adding muriatic or other acids to the bile of the pig, for example, an instantaneous precipitate is formed, of a buff-yellow colour, while the same substance, added to that of the sheep, very slowly produces a dark dull-green precipitate. The same difference in the rapidity of chemical action is observed, when the analysis is carried on by means of acetate of lead,—thus forming a very decided distinction between this secretion in different animals.

The consistence of this fluid does not differ, I believe, remarkably amongst the lower animals, if we except that tenacity

which depends upon the quantity of mucus contained,—the latter substance often being very abundant, at other times bearing only a small proportion to the rest of the biliary matters. The structure of the gall-bladder is different in different animals; that of the sheep, for example, readily permitting the transudation of the bile, while that of the ox completely retains it. Albinus believed that the gall-bladder secreted a bile of its own, chiefly from contemplating its honey-comb structure; but this opinion is not, I believe, entertained in the present day. As observed in the human subject, under the usual course of *post mortem* investigation, the bile presents almost every degree of consistence, being at times perfectly limpid, at others, so tenacious as to admit of being drawn out into threads, in which state it has been described as resembling frog-spawn. On one occasion, I observed it so nearly solid as to take, in some measure, the shape of the gall-bladder containing it.

The average quantity found in the gall-bladder has been stated by Dr Saunders to be one ounce. I believe half that quantity will be nearer the truth. The greatest quantity I have ever seen is two ounces. If, however, we admit into our calculations those extraordinary instances where the quantity has exceeded two Scottish pints, and, according to some continental authors, even a far greater quantity, the average might be placed somewhat higher; but as these form very rare exceptions, they are, I think, better left out of consideration.

The taste of bile is intensely bitter, followed by a slight degree of sweetness, which depends, according to M. Demarcay, on the manner in which it is applied to the palate and tongue, and the greater or less degree of sensibility in these parts, and also on the consistence and size of the morsel tasted. That form of bile named picromel, owes, I believe, its sweetness to the portion of acetate of lead which the sulphuretted hydrogen is insufficient to remove.

The prevailing idea, that bile is a substance which readily undergoes putrescence, has been shown by Dr Saunders to be incorrect, and such has been my own observation. On the other hand, in two persons who had died of fever, and in both of whom the quantity was large, the contents of the gall-bladder were decidedly putrid twenty-four hours after death. The weather was, however, at the time, unusually hot. Dr Davy's remarks regarding the non-existence of free carbonic acid in this secretion, are, I think, perfectly satisfactory, and I merely allude to them. (Researches Phys. et Anat. Vol. ii.)

The presence of a sediment or of grains is extremely common, and is not, I believe, an unhealthy indication, except where these bear a very large proportion to the rest of the fluid. In one in-

stance, these grains appeared to have a nucleus, and, I might almost say, were incipient calculi. It seems probable that this may be the origin of those concretions which are formed of successive layers of biliary matter. In the rabbit, the coats of the gall-bladder are perfectly translucent, and easily admit of these particles being seen floating in a clear yellow liquor.

Considering that my own opportunities of observation were somewhat limited, I collected more than 140 cases in which the biliary secretion had been observed by Bright, Annesley, Thenard, Andral, Orfila, Chevalier, Hodgkin, Graves, and Stokes, and many others, in which the disease under which the patient had laboured, the age, sex, and condition were ascertained, as well as the physical characters of the bile, and where I could meet with it, the chemical composition. But the remarks of all these, except a very few by Orfila, Chevalier, and Thenard, were too general and vague in their nature, to afford more than occasional corroboration of my own remarks. The necessity of possessing some practical form of general analysis, before we can attempt any classification of morbid appearances, must therefore be obvious, and this, I hope, in the ensuing observations on the chemical nature of the bile, to be able to afford.

Thenard has remarked, that in a case of diseased liver, the quantity of biliary matter was small, but little can be deduced from a single observation. I have, however, remarked, that where the patient has been long wasted by disease, especially by *phthisis pulmonalis*, the contents of the gall-bladder have been little else than colouring matter and mucus. In several cases where the vitiated condition of the bile has been very evident, the red colouring matter described by Muratori and Collard de Martigny has been so abundant as to give a brick-red colour to the bile. It somewhat resembles blood, but boiling and the addition of alcohol do not alter it. It is most easily obtained from the bile of the pig by precipitating the biliary matter by means of sugar of lead, and then passing sulphuretted hydrogen through the fluid. The red substance is then easily distinguished by its conspicuous colour, but is in very small quantity.

The relation which, according to Muratori, exists between the quantity of soda and that of cholesterine, has hardly been verified by myself. The cholesterine is most abundant in human bile, while the soda is, I believe, far more abundant in that of the ox, in which, as in all the lower animals except the dog, the cholesterine is in very small quantity. I have observed in some instances, that human bile, on being exposed to the air, has become covered with scales of cholesterine, though the quantity of soda must have remained the same as at the first. Hence it would appear that the use of the soda is not merely to hold in solution the

cholesterine. With regard to the latter substance, out of twenty-five cases examined by myself, five only contained cholesterine, and of these five, two were undoubtedly healthy. With these remarks, I shall now pass on to the consideration of the chemical nature of the bile.

In considering the chemical nature of the biliary secretion, it is not my intention to offer an exact analysis of this substance, but rather to state the results of my own experiments, which have been conducted on a somewhat extensive scale. Before detailing any individual experiment, however, it will be necessary to glance at the commonly received opinions regarding this substance. The saponaceous properties of bile were generally admitted; such at least was the opinion of Boyle, Boerhaave, Fourcroy, Van Boehan, Ramsay, Verheyen, Cadet, and others, until a modification of this opinion was introduced by Thenard. Its physical properties seem alone sufficient to establish this opinion. Its homogeneous nature, its viscidity, its great solubility in water, its attraction for moisture, as well as its well-known property of removing grease spots, and still more the chemical experiments of M. Demarcay, and those made by myself, all tend to prove that it is really a soap having soda for its base.

Before entering on examination of the views of Thenard, I shall allude to a substance contained in the bile, and mentioned by Fourcroy as a substance resembling albumen, considered by Dr Baillie as analogous to white of egg, and by Dr Bostock as a substance intermediate between albumen and mucus. Dr Powell also believed that the bile not only held this substance in solution, but exerted such an influence upon it, as to render it unsusceptible of the action of ordinary reagents. I am not aware that the latter opinion has ever received corroboration; but I have on more than one occasion observed that alcohol was insufficient to precipitate or coagulate this substance, even when used in considerable quantity; yet when this muco-albuminous matter has been precipitated by muriatic acid, and again dissolved in alkalies, it did not exhibit any peculiarities that would entitle it to be considered as a peculiar principle, and in all respects answered to the character of mucus. The saponaceous properties of bile were, as I have said, generally admitted, until Thenard endeavoured to show that this opinion was incorrect, and that bile contained a peculiar substance, (picromel), to which its solubility and tendency to froth when agitated was owing. This opinion was immediately adopted, and the same view is stated by Dr Ure in the following words, "The property which caused bile to be considered as a soap is owing to the soda, and to the triple compound of soda, resin, and picromel." Dr Fyfe also, in speaking of picromel, says that it has the power of dissolving resin; indeed, it is chiefly by its agency that the resin is held in solution in bile, for when unit-

ed with it, it forms a substance, having the peculiar flavour of bile. The picromel, however, is not in sufficient quantity to keep the whole dissolved, so that the free soda must also exert its influence. —And again, " Picromel vel in aqua vel alcoholi solubile est, neque crystallos format, neque fermentationem subit, et simul cum soda et resina bilis fluidam constituit quæ neque acidis nec alkalinis, nec terris iterum separari potest. Picromel igiter sodæ liberæ et albumini junctum, resinam bilis fluidam tenere videtur."

I shall endeavour to show, by a few simple but conclusive experiments, that not only is this picromel improperly regarded as a peculiar principle, but that the resin of Gmelin, the picromel of Thenard, and the " choloidique et choléique acides" of Demarcay are in fact one and the same thing.

The analyses furnished by the different chemists on bile are extremely complicated, and the substances found by them very numerous; that of Gmelin containing twenty-seven different matters as entering into the composition of bile. This, however, includes the oleic and margaric acids, and cholesterine, which are not noticed by Thenard. M. Demarcay, in his paper on the bile, (Annales de Chimie et de Phys. Tom. lxvii.) has presented us with views upon this subject, which are, I believe, altogether original; and it is from the perusal of his paper that I was led to make the experiments that follow; but while his analysis is one of considerable value, it appears to me open to certain objections, which I shall point out.

His analysis is as follows;—1. a substance soluble in water azotized; 2. a substance insoluble in water not azotized; 3. taurine; 4. chloride of sodium. This general division (for it does not appear to be the intention of the author to offer an accurate analysis) is, I believe, incorrectly stated as regards the bile of man in the following points. In the process of boiling the bile with acids, there is a peculiar volatile principle evolved during the first part of the ebullition. This is mentioned by Gmelin as a substance having the odour of musk, and by Berzelius and others as " a volatile odoriferous principle." It is at the same time remarked, that " son existence n'est conclue, au reste, que l'odeur exhalé par l'eau qui passe à la distillation." This odoriferous principle is, however, so constantly evolved from the bile of all animals which I have examined, and its smell is so penetrating and peculiar, that it cannot with propriety be omitted. We must therefore add to the substances enumerated by Demarcay " a volatile odoriferous principle."

The two last substances mentioned are taurine and chloride of sodium. I have examined in a considerable number of cases the bile of the human body, but in no instance have I obtained a trace of taurine; while, therefore, it is properly named as a constituent

of the bile of the ox, cat, and those animals in which it has been found, it cannot be reckoned among the components of human bile. The last substance mentioned is chloride of sodium. I am at a loss to know why this salt should be noticed, and the other inorganic saline matters omitted, more especially as the greater proportion of this salt must, from the nature of M. Demarcay's experiments, be regarded as a product rather than an educt. Thenard regards chloride of sodium as forming a constituent of bile in its normal condition. The phosphates of lime, potassa, and soda, and many others, have been shown by Gmelin and others to be present, and the former of these, in particular, I have had occasion to notice frequently. The results of the analysis of M. Demarcay may be therefore more correctly stated, as 1. a volatile odoriferous principle ; 2. a substance soluble in water azotized ; 3. a substance insoluble in water not azotized ; 4. inorganic salts ; 5. taurine. But I shall have occasion to differ still more widely from M. Demarcay in the subsequent remarks.

I shall detail the following experiment, which is nearly that of M. Demarcay, the only difference being, that he made use of the dried alcoholic extract, and I of the fresh bile.

Experiment 1.—I took a quantity of ox bile, and after precipitating the mucus by means of alcohol, I added a portion of the hydrochloric acid of commerce. A precipitate of a yellow colour was produced, but a complete separation of the biliary matter did not take place till heat was applied. I placed the whole in a flask, and boiled it for two hours. As soon as ebullition commenced, a peculiar and offensive odour was given off, but after some time ceased. A green oily matter was seen collecting on the surface of the fluid, which gradually sunk to the bottom of the vessel, causing such violent succussions as to render great caution necessary in boiling. This green matter gradually assumed a darker colour and greater consistence resembling pitch, while the fluid became clear and had a purplish tinge. The concrete mass was then washed in distilled water and dried ; it was then of a dark-olive brown colour, and easily pulverized. A portion of this was dissolved in alcohol, and agitated with ether, in order to remove any portion of oleic or margaric acids. It was then filtered and dried on a water-bath, when a mass of shining appearance and brittle resinoid fracture was the result, which, when pulverized, was of a yellow colour. The clear fluid from which the pitchy mass had been separated was then placed on the sand-bath, when, after a short time, globules of an oily nature and of a reddish-brown colour were observed to deposit themselves on the sides of the vessel and on the apices of the crystals of chloride of sodium, which were now abundantly formed. The whole then presented a thick mass of a dirty colour, in which nothing could very dis-

tinctly be made out. I next added strong alcohol in large quantity, when after some time a deposit of beautiful crystals were observed, which proved to be sulphate of lime and taurine, or "asparagine biliaire."

Repeating this process on the bile of man and other animals, constant and remarkable differences are observed in them all; but as this does not immediately bear on my subject, I shall omit the consideration of all except that of man. If the foregoing experiment be made on the bile of the human subject, the smell given off on boiling is more penetrating and offensive, while the pitchy precipitate is not in general so concrete. The liquor from which it is separated differs remarkably in colour, resembling port wine in appearance. The resinous matter obtained by the preceding process is of a brighter colour; the proportion of chloride of sodium is less, and in no case have I observed taurine. These remarks only apply to bile of healthy characters, of which I consider the port wine-coloured fluid a very good criterion. In diseased bile the solid precipitate is often in the form of grains, and the fluid is like dirty water. The bile of children differs from that of adults, in having the precipitate less solid and the fluid much paler in colour.

We may now briefly examine the nature of the chemical action observed in this analysis by acids. As bile is perfectly soluble in alcohol, the first step taken, viz. the precipitation of the mucus, needs no remark. The next precipitate is the dark pitchy mass, which on analysing will be found to consist of green colouring matter, a large portion of resin, and some soda, which even the boiling with acids does not immediately remove; possibly the pitchy nature of the mass presenting a mechanical obstruction to the chemical action. The fluid portion of a purplish colour is a solution of resin discoloured by the heat employed, and does not differ in its nature, as I have satisfied myself from that at first precipitated. As the evaporation proceeds, the whole of this resin is deposited, and the use of alcohol readily separates the crystalline portion. M. Demarcay considers that the two precipitates are different, and has named the one "choléique," the other "choloidique acide." This opinion I at first adopted, but have since abandoned. If we take a portion of the first precipitate, and place it on a water-bath with concentrated acid and digest it some time, it will thus treated, assume all the characters of the second precipitate mentioned in the experiment, and a portion of chloride of sodium, and taurine when it exists, be obtained. It appears to be of little moment which acid is employed, as the chemical action of them all is the same. There is, however, an objection to sulphuric, as during the application of heat it blackens the whole mass, so as to render it obscure. It is stated by Demarcay, that

acetic acid does not produce decomposition. There is, however, an immediate change of colour produced by its admixture with bile. The experiments made by Berzelius were conducted by means of this acid; and it was by its aid that his observations on the fatty acids which he termed "cholenique" and "fellique" were made. Hence, I conceive this remark of M. Demarcay to stand in need of some explanation. The taurine mentioned as obtained in the foregoing experiment is best procured from the bile of the ox, in the manner recommended by M. Demarcay. The process given by Berzelius is much less eligible. I believe it is entirely wanting in the bile of man, and I have not hitherto observed it in that of the pig. It has little taste, being neither sweet nor saline, but having a pungent flavour. It is easily dissolved by nitric acid, and, when burned in the platinum spoon, gives a large volume of carbonaceous matter. If the heat be very gradually applied, it assumes the appearance of a dark-brown oil.

The action of the salts of lead upon bile is precisely analogous to that of the acids, and the conclusions arrived at by the aid of the one may be equally obtained by the use of the other. Cadet appears to have been the first who observed that, after adding sugar of lead to bile, by which a copious precipitate is produced, there yet remains a resinous substance in solution. This substance was described by Thenard under the name of picromel, and has since been the object of attention to a great number of chemists, especially to Gmelin. Berzelius, indeed, expresses his suspicion that the resin, picromel, and biliary matter, are not distinct substances.

Experiment 2.—I added sugar of lead to ox bile, by which a copious precipitate was immediately produced. I then passed sulphuretted hydrogen gas through the mixture, filtered and evaporated on a water-bath until I had obtained the extractiform mass of Berzelius, which was sweet and bitter to the taste, and answered to the description of picromel of Gmelin. This I dissolved in alcohol, when a clear yellow fluid was observed, with a white flocculent substance at the bottom of the vessel, being that portion of lead not removed by the sulphuretted hydrogen. The yellow fluid was then evaporated to dryness, and when pulverized was nearly white, resinous in its character, bitter and sweet to the taste. During the pulverization it produced great irritation of the nares, and the lips tasted excessively bitter, as when aloes are pulverized. After some hours it had attracted such a degree of moisture from the air as to have again become nearly fluid.

In the first part of this experiment the precipitate caused by the sugar of lead is precisely similar to that caused in the former experiment by the muriatic acid, being a heterogeneous mass of colouring matters, biliary matter, and soda; that left in solution

being exactly the same substance which is obtained at the latter part of the evaporation of the acid, but still in combination with oxide of lead. It is worthy of remark, that the lead is not wholly removed by the action of the sulphuretted hydrogen, nor yet entirely by the alcohol. I have already expressed my opinion, that the ordinary action of these chemical agents is modified in all probability by the animal matters existing. I have, in experiments made on this point, satisfied myself of the insufficiency of sulphuretted hydrogen to remove the lead; the solutions through which it was passed remaining colourless after its repeated employment, when, by dissolving the dried mass in strong alcohol, a large portion of lead has been separated, some even yet remaining in combination with the resin. It is this circumstance, I believe, which led Gmelin, Thenard, and others to speak of picromel as a peculiar biliary principle. Dr Fyfe observes, that it is necessary "to employ the sulphuretted hydrogen very repeatedly, and even then a small portion of lead will still remain." Demarcay complains of the same difficulty, and uses strong alcohol; but this I have shown is still insufficient. The following simple experiment suggested itself to me, which not only proves the picromel to be a nonentity, (or rather two substances in combination with each other,) but completely obviates the difficulties just described.

Experiment 3.—Having obtained, by means of the salts of lead, the usual form of picromel, or rather what is more convenient, the extractiform mass of Berzelius, I dissolved it in tepid distilled water, and added carbonate of potassa until the effervescence at first produced had ceased. As soon as the liquor had become clear, the carbonate of lead was seen lying in large quantity at the bottom of the vessel. I then carefully filtered the fluid, which had a most nauseous taste, being a solution of resin and carbonate of potassa; and evaporated it until the saline matters began to crystallize, and the mass to assume a resinous tenacity. I then kneaded the whole in weak nitric acid, and once more evaporated to dryness, and added strong alcohol, which dissolved the resin, leaving crystals of nitrate of potassa on the filter. On evaporating the alcoholic solution I obtained pure resin, if we except a trace of soda which still obstinately remained, but which may be removed by a process to be presently noticed. This experiment is perfectly satisfactory, the carbonate of potassa completely removing the lead, and the nitric acid, while it has no action on the resin, is at the same time sufficient to remove the potassa.

We have now only to prove that the picromel of Gmelin and Thenard has no solvent power, and is itself insoluble, and that the resin observed in the first precipitate by muriatic acid, and that obtained in the second part of the process, are identical with each

other, the solubility of the one simply depending on the soda with which it is combined.

Take a portion of picromel, dissolve it in tepid distilled water, add *guttatim* strong nitric acid ; very soon snow-white flakes are observed to form, which gradually unite, adhere to the sides of the vessel, and become slightly yellow. Filter and dry the solid matter, gently heating it when you will have obtained a perfectly insoluble resin. Evaporate next the fluid, and nitrate of soda is found remaining, Take the " azotized soluble substance" of Demarcay, and perform the same experiment, and the same will be the result. The reason that the resin first found is soluble is merely from its containing soda which the high temperature and more concentrated state of the hydrochloric acid afterwards separates. Thus, not only does the substance named picromel vanish, but also one of the principal constituents of M. Demarcay's analysis, " the soluble azotized matter." It may require some apology that I speak with so much decision on a point upon which a different opinion has been entertained by men so eminent as Thenard, Gmelin, Chevreul, Chevalier, Lassaigne, Tiedemann, Fromhertz, Gugert, and others ; but the simple experiments I have detailed do not appear to me to admit of doubt, and remain open for investigation by those who may not feel inclined to admit the views I have entertained.

I have pointed out in the commencement of my paper the necessity there is of arriving at some general analysis of which the pathologist may avail himself, in order to form any certain conclusions as to the healthy or morbid state of the biliary secretion. I beg to offer in conclusion, the following analysis, which has been the result of my own experience; but to which I was led by the perusal of M. Demarcay's excellent paper, and which appears to me to meet the object in view. It is calculated for the examination of the contents of the gall-bladder after death.

Analysis of the contents of the Gall-Bladder.—Empty the contents of the gall-bladder into a measure glass, (this is best done by dissecting out the gall-bladder,) and, having noted the quantity, dilute it if requisite until it be limpid. Add to this sufficient alcohol to precipitate the whole of the mucus, which, being separated, next add concentrated muriatic acid, stir it assiduously for some time, and leave it for an hour, when, if there be any cholesterine, it will be seen floating in thin scales upon the surface. This may be elevated by the aid of a piece of blotting-paper. Next place the whole in a flask, and boil gradually for an hour and a-half to two hours. A blackish mass and a port-wine coloured fluid will be obtained. Separate the two, evaporate the fluid until the resin with which it is coloured is deposited together with the chloride of sodium. If it be desirable to make the analysis

very complete, it will now be advisable to add very strong alcohol. This dissolves the resin, purifies the chloride of sodium, and allows any other saline matter that may be present to crystallize. Next dissolve the black mass first obtained in warm distilled water. A milky fluid of a greenish white colour is produced. Allow this to remain some hours, when an insoluble dark-green matter will be seen to have separated at the bottom of the vessel. Pour off the milky fluid, and wash the insoluble green matter in tepid distilled water, adding the washings to the rest of the milky fluid. To the latter next add strong nitric or muriatic acid (muriatic will be the best) until the opaque fluid is perfectly clear, and there is a precipitate of a resinous matter. This precipitate is to be washed, dried on a water bath, and added to the resin first obtained. If it be desirable to proceed with accuracy, the fluid from which the second portion of resin was obtained should be evaporated, by which a small quantity of chloride of sodium will probably be obtained, which is to be added to the portion first procured.

We have thus obtained the relative proportions of the four following substances. 1. a dark green insoluble matter; 2. resin; 3. cholesterine; 4. chloride of sodium and other salts.

There is no objection in this analysis to the use of the ordinary muriatic acid, which contains a certain proportion of sulphuric acid, as the action of all these acids is the same as regards the bile; and if, as is usually the case, the bile contains a portion of phosphate or carbonate of lime, it is decomposed by the sulphuric acid, and the sulphate thus formed from its insolubility is very easily recognized.

By performing this analysis a few times with healthy bile, a mean would be very easily obtained of the relative proportions of these substances, from which comparisons might as easily be made in cases of disease. The process is simple, yet sufficient; and a reference to the former part of my paper will show how much more valuable the remarks of Andral, Bright, Annesley, and others would have been, had they been the result of some such definite examination as that I have now suggested.

When I first turned my attention to this subject, I looked in vain through the different chemical works for some such analysis as that I have given for my guide. It would indeed have been of the utmost value; and I have therefore endeavoured to afford to others who may pursue this line of investigation that assistance which was necessarily denied to myself.

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